1850MNRAS..10..100A

The Calcutta Public Library.
The Society at Zurich.
The Editor of the Athenæum Journal.

R. Abbatt, Esq.
G. B. Airy, Esq.
M. J. J. Bayer.
M. J. B. Biot.
H. Blunt, Esq.
Professor W. C. Bond.
Don Sanchez Cerquero.
Professor Cowper.
Professor Chevallier.
Professor De Morgan.
E. J. Dent, Esq.
S. M. Drach, Esq.
John Drew, Esq.
Professor Encke.
Executors of the late Father De Vico.
Mons. V. Fasel.
C. Frodsham, Esq.
W. Galbraith, Esq.
Lieut. Gillis, U.S.N.
James Glaisher, Esq.
Dr. B. A. Gould.
Peter Gray, Esq.
J. Herapath, Esq.
Sir J. F. W. Herschel, Bart.
Luke Howard, Esq.
Peter Legh, Esq.
E. J. Lowe, Esq.
Dr. Lee.
MM. Levy & Lewandowski.
Sir John Lubbock, Bart.
Professor Mädler.
J. Martin, Esq.
J. Nasmyth, Esq.
Henry Perigal, jun. Esq.
The Baron Plana.
Professor Plantamour.
Professor Quetelet.
Lieut. H. Raper, R.N.
Capt. Sir John Ross, R.N.
M. Carl Rümker.
Dr. Rutherford.
Professor Schumacher.
Arthur Scratchley, Esq.
Capt. Shadwell, R.N.
Rev. R. Sheepshanks.
M. F. G. W. Struve.
R. Taylor, Esq.
B. Vulliamy, Esq.
A. D. Wackerbarth, Esq.
Mr. J. Williams.
J. W. Woollgar, Esq.

Address delivered by the Astronomer Royal, President of the Society, on presenting the Honorary Medal of the Society to M. Otto von Struve.

It has been stated to you, Gentlemen, in the Report of your Council, that the Medal of the Society has been awarded to M. Otto von Struve, for his paper entitled, "*Bestimmung der Constante der Präcession mit Berücksichtigung der eigenen Bewegung des Sonnensystems*," communicated to the Academy of St. Petersburg on the 19th of November, 1841, and printed in the third volume of their *Mathematical and Physical Transactions*, sixth series; and it is now my most agreeable duty to point out to you the state of the questions to which this paper relates, the need for a new investigation, the skill and care with which M. O. Struve's investigations have been conducted, and (in a word) the claims which the selection of the subject and the mode of treating it have presented for the honourable notice of the Society.

1850MNRAS..10..100A

The subject of precession, generally, has always appeared to me one of the highest interest in physical astronomy. Its discovery, as an astronomical fact, dates from the earliest age of scientific astronomy. Its existence was recognised, and its quantity tolerably well measured, by the astronomers of the succeeding ages. But century after century rolled away, and no human being entertained the smallest idea of its cause. It stood as an anomalous fact, perfectly certain in its existence, perfectly clear in its law, but unconnected in its form, or in its probable causes, with any other phenomenon known to man. The establishment of the Copernican theory did not appear likely to aid at all in its explanation. Vortices might be conceived which would carry planets round the sun, but no vortex was devised which could give to the earth's axis the gradual change of position required to explain the fact of precession. At length Newton,—thoroughly imbued with the knowledge of the Galilean mechanics, powerful in the use of the new calculus which he had already invented, ready to invent newer methods whenever they should be required,—examined in a mechanical point of view the principal laws of the planetary motions, and the principal perturbations of the moon, and explained them fully by his theory of gravitation. But a part only of the theory was required in these explanations. It sufficed, in regard to the attractions, to consider the attraction and the liability to attraction, of every body in the solar system, as being fixed in its centre. It sufficed, in regard to the laws of mechanics involved in the investigation, to consider the moving bodies as points. Had Newton stopped here, the explanations of precession, of the figure of the earth, and of the tides, would have been lost. At this distance of time I cannot refuse my admiration to the generality of conception which suggested to him that the centre of a planet can have no special virtue, except from the combination of the actions of all its parts, nor to the undeviating firmness with which he retained this conception, nor to the mathematical sagacity with which he perceived that one of the consequences of this conception must be an ample explanation of precession and the related phenomena. But in what form was this explanation to be put? At the present time, the explanation of precession, from its mechanical causes, in a form which shall be extensively intelligible, is one of the most difficult practical problems that I know. In the first place in which Newton treats of it, he derives its explanation, by a very simple analogy, from that of the motion of the plane of the moon's orbit; and I believe that this is still the easiest illustration that can be offered. In the second place, where he makes it a matter of calculation, he abandons that form, and treats it (as it ought finally to be treated) as a case of the theory of rigid bodies. A small error was made in this theory; but it was unimportant. Regarding its object as being this, to show that the phenomenon, generally, was explained, and that its magnitude was computed as nearly as could be expected, it was perfectly successful. And I have no hesitation in expressing my belief that, if I had lived in

1850MNRAS..10..100A

that critical time of science, the explanation to which I should have appealed as establishing the truth of the theory of gravitation, would have been the explanation of precession.

Among the applications of our knowledge of precession, the most striking, perhaps, to ordinary imaginations, is that which connects it with the broader and coarser features of very ancient chronology. The stars over which the colures have passed in succession, as one century after another has slipped away, may well be described in the words of a modern poet, applied to a different kind of memorial,—

> "Like dials, which the wizard Time
> Had raised to count his ages by."

In some instances, undoubtedly, this use of the theory has been abused; in all it is liable to considerable uncertainty. Nevertheless, it is one which sometimes gives us information when all other sources fail. And nothing appears more marvellous than that it should be possible, from a single well-authenticated shepherd's observation of the heliacal rising of a star, to infer the date with a precision much exceeding that of many of the records of history.

To the practical astronomer, however, of the present day, the exact knowledge of the law and magnitude of precession is more necessary for another reason. Not a single observation can be properly reduced, not a clock-error can be investigated, not a place of sun, or moon, or planet, or comet, can be brought to a state in which theory can be confronted with observation, without an accurate knowledge of precession. And it is this, in fact, which has given rise to the necessity for repeated investigations of the numerical value of the constants of precession.

In this, as in all other cases of natural philosophy, the more the accuracy of observations is increased, the greater becomes the complexity of the laws of nature which it is necessary to take into account; and the investigation, which at first was intended only for the purpose of correcting the numerical coefficients of a known theory, may lead to the discovery or verification of a subordinate theory of a totally different kind. We shall find that this very circumstance occurs in the investigation which is now before us.

The determinations of precession to which, by common consent, the astronomers of Europe had referred before M. O. Struve's investigation was undertaken, and which, in fact, are most generally used at the present time, are Bessel's. They were made at two successive steps. The first of these is detailed in the "*Untersuchung der Grösse und des Einflusses des Vorrückens der Nachtgleichen,*" a paper honoured with the prize of the Berlin Academy; its last alterations are dated 18th August, 1815. The subject of precession is treated in this paper by a comparison of Bradley's places of 2429 stars in north polar distance, and 2278 stars in right ascension, with the places in Piazzi's two catalogues. It is needless for me to trouble you with details of the division of the heaven into

1850MNRAS..10..100A

zones, and of the subdivision of each zone into portions, or of other devices suggested by the author's well-known skill. But it is important to remark that Bessel fully recognised the difficulties introduced by the proper motions of the stars and the probable movement of the solar system, and that he pointed out a certain degree of regularity in the discordances of results, which however, in his opinion, was not at all favourable to Sir William Herschel's supposition that the motion is directed towards the constellation *Hercules*. These difficulties he hoped to overcome by using for the comparison a large number of stars. He also remarked critically on the possible errors of Piazzi's catalogue, as inferred from his discordant determinations of the obliquity at the two solstices, and on the effect of those errors. And he treated the immediate results of his investigation with due reference to the difference between general and luni-solar precession, and founded upon that difference deductions as to the mass of *Venus*.

In the Berlin *Memoirs* for 1818–1819, Bessel gave his catalogue of 36 stars, founded upon his own observations and his own determination of the equinoxes, using Von Lindenau's new coefficient of nutation in the reductions. In the *Memoirs*, 1825, he gave new determinations of right ascension of the same stars, founded on observations made with his new meridian instrument. And in the *Astronomische Nachrichten*, No. 92, he brought together these results, compared them with those which would have been obtained from Piazzi's catalogue if corrected by Von Lindenau's nutation, and used the comparison to correct the coefficients of precession found in the first investigation. And these corrected coefficients are those used in the *Tabulæ Regiomontanæ*, and usually referred to as Bessel's coefficients.

It will be seen from the account which I have given that, though the first determination was made by means of 2400 stars, yet the places at one end of the period are, in fact, carried on by the observations of thirty-six stars; and that the determination is sensibly affected by the use of a coefficient of nutation now generally allowed to be erroneous.

Some time after this, the attention of astronomers was drawn pointedly to the theory of the proper motion of the solar system. The first enunciation of this theory had been given by Mr. Herschel (afterwards Sir William Herschel) in the *Philosophical Transactions*, 1783. In the first place, he had taken Dr. Maskelyne's proper motions of *Sirius*, *Castor*, *Procyon*, *Pollux*, *Regulus*, *Arcturus*, and *α Aquilæ* (all in right ascension, and *Sirius* and *Arcturus* in polar distance), and had shown that all these motions are explained by a motion of the solar system towards *Hercules*. He then took twenty-seven proper motions, given by Lalande for fourteen stars, and showed that twenty-two of them are explained by motion towards λ *Herculis*. He then took forty-four different stars from Mayer, and showed that the proper motions of thirty-two of them agreed with the same hypothesis. In the *Philosophical Transactions*, 1805, he again took up this subject, and he investi-

1850MNRAS..10..100A

gated the pole of intersection of the apparent paths of several pairs of stars. But the most remarkable evidence which he found for his conclusion was thus obtained. The sum of the annual apparent motions of *Sirius*, *Arcturus*, *Capella*, *α Lyræ*, *Aldebaran*, and *Procyon*, was found to be 5″·35. But supposing that the movement of the solar system is towards a point, R.A. 245° 52′ 30″, N.P.D. 40° 22′, and supposing that so much of the proper motions of the stars as is directed towards that point is entirely due to solar motion, then the sum of the residual proper motions is only 0″·96.

It is unnecessary for me to say at length how this theory was supported by some astronomers, as Prevost and Klügel, and how it was doubted by others, as Maskelyne, Burckhardt, and Von Lindenau. Bessel, in the 12th section of the *Fundamenta* (published in 1818), gave a series of numbers defining the directions of the proper motions of seventy-one stars (including all the stars of the *Fundamenta* whose proper motion amounted to 0″·5 annually), and simply remarked on these that they did not confirm Herschel's theory. No important addition was made to the investigation until Argelander took it up, in a paper communicated to the Academy of St. Petersburg in 1837, and printed among their *Divers Savans*, tom. iii. The foundation of this investigation was Argelander's own admirable catalogue of 560 stars for 1830 (published in 1835), and the proper motions which accompany it. Of these 390 were found available for his purpose. He proposed to treat these proper motions by the method of minimum squares; and the first question was, What ought to be selected for the nature of the residual error, the sum of the squares of which for all the stars should be minimum? The quantity which he adopted was the angle (expressed in degrees not numerically exceeding ± 180) between the direction of the star's proper motion and the great circle connecting the star with the pole of solar motion, multiplied by the sine of the star's distance from that pole, without any reference to the amount of the proper motion or to the star's brightness. The result, corrected for some errors, is given in the *Astronomische Nachrichten*, No. 363, the place of the pole of solar motion is found R.A. 259° 52′, N.P.D. 57° 31′. Lundahl (*Astron. Nach.* No. 398), by an application of the same method to a comparison with 147 of Pond's catalogue of 1112 stars, which had not been included in Argelander's catalogue, found results considerably different, especially in declination. It is proper to state that Mr. Galloway, in a paper printed since the appearance of that which is the subject of our medal, has applied Argelander's method to the southern stars; his resulting R.A. is greater, and his N.P.D. less than Argelander's.

The state of the matter when M. Otto Struve undertook his investigation may now be well understood. The determination of precession, though undoubtedly good, admitted probably of improvement. A new element of reduction had attracted general attention; the evidence for its existence, with elements expressible numerically by a measurable or definable quantity, had become too

1850MNRAS..10..100A

strong to be neglected; at the same time that the form of the latest investigations, founding their measures of error on circular angles, and omitting absolutely the consideration of probable distance, might not be satisfactory to every investigator.

The difficulties of this investigation, when fully considered, are almost bewildering. So long as we conceive any one material body to be fixed, our notions of motion, whether of translation or of rotation or revolution, are perfectly clear; but when once we give up the idea of material immobility, we soon perceive that the notion of a zero, either of linear measure or of angle, is a purely metaphysical conception. In applying it to nature we must assume something as arbitrary.

The stars used by M. Otto Struve are 265 double stars, and 74 pairs of stars too widely separated to be included under the same term, of which the right ascensions and north polar distances had been observed by M. Struve at Dorpat: Maskelyne's 36 stars (some of which are included in the former list); 42 circumpolar stars, which had been repeatedly observed with reference to the theory of refraction; and 39 stars not included in either of these lists. The whole number is 400. The places of the stars are not reduced to one epoch, but each star is reduced to the beginning of that year which corresponds most nearly with the mean of its times of observation, Argelander's proper motions being applied as far as they go, and in some other instances proper motions deduced from a first approximate comparison with the *Fundamenta* being applied. The daily reductions of the star-places were made by means of the numbers in the *Tabulæ Regiomontanæ*. The clock-errors were obtained from the fundamental catalogue in the introduction to the *Dorpat Observations*, vol. vi., the place of the equinox being there determined by 216 observations of the sun distributed over eight equinoxes. Ample proofs are given by Mr. Otto Struve of the accuracy of his individual results. The only possible objection which could be advanced to these determinations for the second of the epochs of comparison, is the preponderance of double stars, whose proper motions, in general, follow peculiar laws. Of those here used, however, the vast majority, without doubt, are only optically double, and no such objection applies to them. And in every other respect the accuracy of the determinations is probably equal to that of any mass of star-places that has ever been collected.

The geometrical annual precessions were then computed for every star for three different epochs, namely, Bradley's epoch, 1755; O. Struve's epoch (which, as is mentioned, is different for different stars); and for the epoch half way between them. An approximate formula founded on these gave with facility the whole geometrical precessional motion, accurate to the third power of the time. The comparison of this with the difference between Bradley's and O. Struve's catalogue, gave the proper motion affected by the errors of precession.

And now the metaphysical difficulty to which I have alluded

1850MNRAS..10..100A

comes into play. M. O. Struve, in the first portion of the investigation, eluded a part of the difficulty by assuming as established that the solar system does move towards the point finally indicated by Argelander. Still we have not delivered ourselves from difficulties. The zero of angle in the celestial sphere will evidently change according to the magnitude which we attribute to the apparent motions of stars right and left of that pole of the sun's motion, so far as the apparent movement of the stars depends on the sun's motion. What law applying to these parts of the star's apparent movement,—or, in other words, what law of the distance of the stars,—shall we assume? After showing that the proper motions are really (upon the whole) largest for the brightest stars, M. O. Struve finally determines on adopting the law of distance, as connected with magnitude, given by M. Struve in his introduction to the *Catalogus Novus*. I fully agree with Mr. Main (to whom the Council are indebted for a critical abstract of M. O. Struve's paper), that M. O. Struve has omitted some important criteria of distance, namely, the binary nature of the double stars and the amount of proper motion. And at the same time I agree with Mr. Main's further remark that, under all circumstances, there was nothing better to be done for the mass of stars than to adopt the rule which M. O. Struve has adopted. For the stars of less brightness, the stars of one magnitude are assumed to be at one distance; but the stars of the first magnitude are subdivided into several groups of distances. After all, M. O. Struve has thought it necessary to exclude seven small stars of extraordinary large proper motion, as either having peculiar movements in space too large to be tolerated in such an investigation, or as being much nearer to us than Struve's rule of distances would place them.

Equations were then formed, expressing the numerical discordance of the computed and the observed precessional motion by two symbolical terms: one multiplying a correction of the principal term of precession, the other multiplying a symbol expressing the apparent angular motion of the sun in ten years, as viewed from a fixed star, at the mean distance of stars of the first magnitude, in a direction at right angles to its linear motion. This second coefficient was always affected by the star's assumed distance as denominator.

The equations in this shape, M. O. Struve remarks, would give an overwhelming influence to the bright stars; and, therefore, he reduces their weight by a factor founded on the assumption that the uncertainty on the discordance of the two catalogues for a star of the sixth magnitude is equal to the average proper motion, during seventy years, of a star of the sixth magnitude, and equal to the sun's angular motion in the same time: and he represents the whole probable error, $5''\cdot5$, by the square root of the sum of their squares. For stars of other magnitudes he retains the first term unaltered, but increases the second and third in the inverse proportion of their distances; and thus he obtains a larger probable error or a smaller weight for the equations for the near stars.

1850MNRAS..10..100A

The selection of this form of equations, combined with one of which I shall speak shortly, amounts to this, that upon the whole mass of stars, as far as we are able to form a conjecture of their positions in space, we assume that the algebraic sum of their movements in space estimated in any one direction = 0, and we also assume that the algebraic sum of the geometrical moments of their motions round any one point = 0.

With the equations affected by these weights, he obtains as result, that Bessel's first coefficient of precession in right ascension ought to be increased by $+1''\cdot 65$; that his second coefficient ought to be increased by $+0''\cdot 24$; and that the sun's angular motion in ten years (seen as is above mentioned) is $3''\cdot 15$.

But when these numbers were substituted in the original equations, so as to exhibit the residual errors, it appeared that the numerical quantity for error of observation was too great. A new assumption was then tried, namely, that the probable error of determination is half the value of average proper motion of a star of the sixth magnitude; and the equations were solved anew. The results were very little altered.

The star-corrections in the original investigation of observed places, it is to be remarked, had been reduced with the tables of the *Tabulæ Regiomontanæ*, and, therefore, with Von Lindenau's nutation. But as the investigations of Busch and Peters had now proved beyond doubt that a value of nutation, somewhat different from those adopted in the reductions for either epoch, was nearer the truth, the proper correction was applied for the change. The final result is, that the coefficient of general precession is $50''\cdot 235$.

And, adopting $0''\cdot 2$ as the annual parallax of an average star of the first magnitude, the sun's annual motion is found to be $1\frac{1}{2}$ times the radius of the earth's orbit. This linear motion appears to be less than that of stars in general.

M. O. Struve then enters into an investigation of the correction of Argelander's point, by finding symbolically the variations of the expressions for the effect of solar movement, as depending on the variation of the right ascension and polar distance of that point; the numerical value already found for the quantity of solar motion, and the proportion of distances of the stars, being adopted as known elements. The quantity which is exhibited as error, to be reduced by the method of least squares, is the amount of proper motion in right ascension and in polar distance yet to be explained. The weight given to each equation is the same as that in the former investigations. A point is thus found, several degrees north of Argelander's final point, and having greater right ascension, but not very distant from that first found by Argelander. This investigation appears to me the most satisfactory that has been made. But another investigation, which is subjoined to it, tends in no small degree to injure the credit of this and of every other investigation directed to the same object. M. O. Struve examines the effect of supposing a small constant error to exist in all the decli-

1850MNRAS..10..100A

nations of one of the two catalogues compared; and he finds, that if all Bradley's declinations are diminished a single second, the pole of solar movement will be shifted twenty degrees to the south. I have personally no belief in the freedom of Bradley's declinations from a constant error exceeding a second, even as depending solely on the mean wearing of his quadrant-centre during the time of his observing with it; and I therefore am induced, by M. O. Struve's remarkable conclusion, to look upon all these determinations of the pole of solar movement with doubt, at least as regards the declination of the assigned point.

I have entered into unusual details, Gentlemen, upon this paper, because I felt that without them I could scarcely explain to you its peculiar merits. The problem of the determination of precession, roughly considered, is extremely simple; but, considered with reference to these minute circumstances which are exhibited only in large masses of observation of the most delicate kind, it is extremely complicated. I have no hesitation in expressing my own opinion that M. Otto Struve has grappled with the various difficulties that present themselves, metaphysical and mathematical, better than any other astronomer whose work on the same subjects I have examined. The two investigations which relate to the determination of the direction and magnitude of the solar movement are, in my opinion, very admirable; but the third, which exhibits the amount of uncertainty in the result depending on venial or probable errors of observation, is, in my judgment, even more valuable. I esteem a rational doubter in science as I esteem a Niebuhr in history—as the only person who is likely to supply at last a firm foundation upon which a solid superstructure may be built.

From what I have already said, you will have gathered my opinion that the subject is not yet entirely exhausted. In the first place, I observe, that the remarks upon the effect of a constant error affecting the declinations of all the stars in the same way suggest the propriety of investigating the effect of an error which affects half the stars in one way and half in another; for such an error does actually exist in the reductions of the stars with an erroneous nutation. In the second place, I trust that no great number of years will elapse before it will be practicable to compare the results of observations made at both epochs with circular instruments; for I have little belief that the results of observations with the quadrant can be free from constant error, of magnitude sufficient seriously to vitiate the final inference. In the third place, I should be glad to see the results of comparisons of the observations of stars which are not ostensibly confined to one class, as that of double stars. And since the date of the investigations of M. Otto Struve, more ample means of an unobjectionable character have been collected. For I must remark that a rather unusual delay has occurred in our notice of this paper. This has arisen partly from the delay which usually occurs in the printing and distribution of foreign memoirs; partly from the time which is necessary for thoroughly reading a paper of

1850MNRAS..10..100A

such length; but principally from the occupation of the minds of astronomers, as well within as without the Society, by the remarkable planetary discoveries made in several years past.

But these remarks are entirely prospective, and have nothing to do with our opinion on the merits of the paper before us, or on its claims to the warmest expression of the high estimation of this Society. I am sure, Gentlemen, that those who have been able to follow the account which I have endeavoured to give of the state of our knowledge of the subjects to which it refers, of the skill and caution with which these subjects are here treated, and of the peculiarly valuable character of the results obtained, negative as well as positive, will agree with me, that on two of the most interesting subjects in astronomy (the precession of the equinoxes and the solar movement) M. O. Struve has given to the world a paper of the most important character, worthy of himself and worthy of the name which he bears. On the award of the Council, and I am sure with your full approbation, in the name of the Society I deliver to our Foreign Secretary, to be transmitted to M. Otto von Struve, the Medal of the Society.

The President then, delivering the Medal to Mr. Hind, Foreign Secretary, addressed him in the following terms:—

In transmitting this medal to M. Otto von Struve, convey to him our assurance that this Society, in common with all the astronomers of the civilised world, regard with the utmost interest the investigations conducted by his father, by himself, and by his colleagues, in regard to those points of astronomy which have now for some years been the distinctive objects of pursuit at the Observatories of Dorpat and Pulkowa. And express to him our confidence, that both in his connexion with the astronomical observations of Pulkowa and in his superintendence of the astronomical operations of the Russian Survey, his life will be distinguished by a series of investigations equally interesting and equally successful with that which forms the subject of the present Medal.

The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President:

G. B. AIRY, Esq. M.A. F.R.S. Astronomer Royal.

Vice-Presidents:

Rev. ROBERT MAIN, M.A.
Lieut. HENRY RAPER, R.N.
EDWARD RIDDLE, Esq.
Rev. RICHARD SHEEPSHANKS, M.A. F.R.S.

Treasurer:

GEORGE BISHOP, Esq. F.R.S.